

# THE SOAL-GOLDNEY EXPERIMENTS WITH BASIL SHACKLETON: NEW EVIDENCE OF DATA MANIPULATION

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## INTRODUCTION

The experiments in telepathy carried out by Dr. S. G. Soal with the percipient Basil Shackleton, during the years 1941-3, came to be regarded as one of the most impressive series in experimental parapsychology—a mainstay, even, of the evidence for ESP. In 1960 it was revealed (Ref. 1) that Mrs. Gretl Albert, an agent at Sittings 15 and 16, had alleged following Sitting 16 that she had seen Dr. Soal “altering the figures” several times on the score sheets—the “GA allegation”. Discussion of the affair rumbled on during the sixties, culminating in an ingenious, but unsuccessful, attempt by Dr. Medhurst to refute the allegation by identifying the target sequences (Ref. 2). Subsequently, Drs. Scott and Haskell presented (Ref. 3) strong statistical evidence in support of the GA allegation: specifically, the changing of target 1's into 4's and 5's. A fuller account of the circumstances of the GA allegation may be found in Refs. 2 and 3.

The present paper reports the discovery of new evidence—of a more direct kind than hitherto published—bearing upon the question of manipulation in the target sequences of the Shackleton data, and possibly extending to the Stewart data.† Part 1 relates how this evidence came to light, describing its essential features; Part 2 presents the computer results with detailed analyses; Part 3 discusses possible interpretations of the evidence.

## PART 1 GENERAL: DISCOVERY OF THE NEW EVIDENCE

The controversy over the GA allegation came to my notice through reading the paper by Dr. Medhurst in the March 1971 *SPR Journal* (Ref. 2) in which he described his computer search for the source of the prepared random numbers used in the Shackleton target sequences—and the disconcerting failure of that search, implying (at

† The 1945-9 experiments with Gloria Stewart, described in Ref. 4.

*Proceedings of the Society for Psychical Research* [VOL. 56, PT. 211 the very least) an inaccuracy in Dr. Soal's precise description of his method for obtaining quasi-random digits (viz. final digits of 7-figure logarithms at intervals of 100, digits 6, 7, 8, 9, 0 ignored).

It was in 1960 that my enthusiasm for Parapsychology had been awakened through the chance reading of Soal and Bateman's *Modern Experiments in Telepathy* (Ref. 4.). Now the research reported therein was being called in question, and I felt a special interest in seeing the matter resolved, intensified by a certain consideration to be mentioned anon.

Accordingly, in April/May 1971 I wrote a computer program to try out some ideas which had occurred to me for extending and modifying Dr. Medhurst's search technique, and applied it to sequences quoted in Ref. 2. Ways in which Soal might legitimately have used Chambers's Logarithm Tables—more or less consistent with his description—are almost limitless. Some selection procedures I examined included: sequences taken in reverse order; intervals greater than and less than 100; digits 6, 7, 8, 9, 0 treated "mod 5" (i.e. remainder on subtracting 5); 6-figure logarithms; digits selected by reading *across* an intermediate pool assumed compiled by column (of unknown length). Two-step procedures such as the latter are discussed by Dr. Pratt in Ref. 5.

In addition, my program allowed an occasional discrepancy in the comparison process, in an effort to identify the target sequences *whether manipulated or not*. None of these efforts met with any success however, and I had reluctantly to abandon the quest.

The next development came in February 1975, while studying the paper "Fresh Light on the Shackleton Experiments?" (Ref. 3.) in which Drs. Scott and Haskell presented an impressive statistical case in support of the GA allegation. Impressive . . . yet hardly *conclusive*. One longed for a conclusive settling of the matter—either way. Re-reading the Medhurst paper (Ref. 2) at that time, my attention was arrested by a certain paragraph (p. 53/54), and this set me off on a fresh trail beginning with a mathematical analysis of sequences derived from final digits of logarithms (Ref. 6). Such sequences have long been known to be *pseudo-random*, but the precise nature of the non-randomness seems not to have been generally realised: systematic effects, apparently characteristic of a wide range of selection procedures, and immune to the usual tests for non-randomness. This result also seemed to offer a possible explanation† for the huge ABA deficit (around 40 per cent) in the Stewart target lists.

At this stage I contacted Mrs. Goldney, who very kindly granted

† Not so far substantiated.

me access to the Shackleton records as well as the Scott-Haskell computer print-out (Ref. 3, p. 47). I embarked upon an extensive series of handcounts on the target lists (computer time not being then available), seeking the characteristic "logarithmic" non-random effects referred to above.

While engaged on these handcounts, I recognised what I took to be one of the target sequences quoted in Ref. 2. On checking, however, I discovered that the sequences came from different sittings, *though matching to the extent of 19 digits*—far beyond what could reasonably be attributed to chance. Within the hour a second long repetition (24 digits) had caught my eye, involving the same two sittings (Sittings 4 and 6).

I subsequently sought other instances but, finding none, concluded that the duplications were probably isolated curiosities, perhaps occasioned by Soal's inadvertently re-entering his pool at a previous point. Moreover, there was nothing suspicious about the distribution of hits in the duplicated sequences. I therefore resumed my handcounts—whereupon I started coming across long repetitions again: four more in all. At least one could say that the *repetitions* must be free from manipulation in the target digits (otherwise they would not appear as repetitions), with a scoring rate comparable to that in the data as a whole.

I reported these findings to Dr. Pratt, who encouraged me to undertake a systematic computer search—suspecting that the duplication might be extensive and that, if so, a pool of target sequences true to some unknown source would be formed, with the same potential for exonerating Soal as though they had actually been located in Chambers's tables.

The initial computer search, for repetitions of 12 or more digits, covered Sittings 1–24 (certain "counters" data excluded). The duplication proved *not* to be extensive, but a very suspicious feature came to light. Some of the repetitions were not exact but exhibited occasional "extra" (or extraneous) digits, as though digits had been inserted into one of the pair of sequences (or omitted from the other), *and these extra digits showed a marked tendency to correspond to hits*. I first observed this effect in Sitting 23 (sheets 3, 5) through studying the *continuations* of the duplicated sequences: five single extra digits became evident at five-digit intervals. On checking the distribution of hits I saw, to my dismay, that *every one of the five extra digits corresponded to a hit*.

This discovery prompted a fresh attempt at tracing the origin of certain target sequences, in the hope of clarifying the anomaly of the

*Proceedings of the Society for Psychical Research* [VOL. 56, PT. 211 extra digits. Soal had specified (Ref. 4, p. 137) that Tippet's random number tables had been used on three occasions (Sittings 24, 25, 26). Tippet's tables constituted, in effect, a ready-made pool (albeit a large one) which Soal might conceivably have used in a fairly direct manner. Accordingly, I selected two bold-looking sequences (one each from Sittings 24 and 25) consisting of five 4's and an embedded 2, intending to scan Tippet's tables by eye—when I realised that the continuations of the sequences were the reverse of each other! Examination of the target lists for Sittings 24 and 25 revealed a systematic near-reversal arrangement with respect to the record sheets. Near-reversal is the crucial point, for some 15 digits became apparent (not at noticeably regular intervals however), all but two corresponding to hits. Moreover, the suspicious hits virtually accounted for the "ESP" effect in the subsitting concerned, Sitting 25 (sheets 1, 3, 5, 7). The effect observed in Sitting 23 was thus devastatingly confirmed.

Other, isolated, instances were spotted by eye, including three cases bridging the Shackleton and Stewart data. Occasional instances of multiple duplication were also encountered. Subsequently I modified my computer program to locate "interrupted" duplicated sequences, covering the whole of the Shackleton prepared random numbers together with a proportion of the counters data. Although not a great many more cases thus came to light, the procedure made possible an objective selection of cases and assessment of significance.

About three in four of single extra digits were found to correspond to hits, the odds against such a high rate occurring by chance being thousands of millions to one. Furthermore, when the suspicious "extra digit" trials are discounted the scoring rate in the record sheets concerned falls to chance levels, leaving an insignificant "ESP" effect.

The computer search also revealed some totally unexpected instances of duplication:

- (i) An 18-digit repetition, in the target lists allegedly prepared by Dr. Wassermann, of a Soal PRN sequence (thus raising the question of possible substitution of sheets by Soal).
- (ii) Repetition of complete 25-digit columns (in reverse) connecting sittings in which the target lists were *allegedly* compiled from two different sources, Chambers's and Tippet's tables.

It should be understood that one is limited, in the analysis of extra digits, to those record sheets which happen to exhibit duplication: less than 20 per cent of the total prepared random numbers. Extension to

other record sheets, and to the Stewart data, could only be achieved by identifying the source of the target sequences.

If the computer search in the logarithm tables had failed because of the presence of manipulation, then it seemed one might fare better with duplicated sequences, assumed manipulation-free. I therefore selected a number of suitable sub-sequences and carried out a further computer search, but this again drew a blank—as did a search on sequences taken from the Stewart data.

Incidentally, a preliminary computer search through the 41600 digits comprising Tippet's tables, in four directions, also failed—but here again there are an almost unlimited number of legitimate selection procedures.

The findings outlined above will now be presented in detail. If desired, the following technical section may be omitted on a first reading. However, to gain an idea of the incidence of long duplicated sequences among the prepared random numbers, and of the nature of the "extra digit" effect in interrupted duplicated sequences, it is recommended that Tables 1 and 3 be studied (in conjunction with the list of abbreviations and opening paragraph). The probability values in Tables 6 and 7 should also be noted, while the final paragraph of Part 2 is especially relevant.

## PART 2 TECHNICAL: COMPUTER RESULTS AND ANALYSIS

Abbreviations: PRN—prepared random numbers  
WN—PRN compiled by Dr. Wassermann  
E—chance expectation  
BS—Basil Shackleton sittings  
GS—Gloria Stewart sittings  
DS—duplicated sequence  
IDS—interrupted duplicated sequence  
ED—extra (or extraneous) digit  
REV—reversed (of sequences)

The Shackleton data comprises 40 sittings divided into subsittings according to varying experimental conditions, principally: telepathy/clairvoyance, normal/rapid/slow rate of presentation of targets, PRN/counters randomisation procedure for target lists. The Soal-Goldney Report (Ref. 7) records significant scoring in 40 "telepathy"

*Proceedings of the Society for Psychical Research* [VOL. 56, PT. 211 subsittings (six of them in two modes); with one exception the scoring occurs in "+1" (precognitive) or "-1" (postcognitive) modes for normal rate, "+2" or "-2" modes for rapid rate, "0" mode for certain cases where Shackleton was urged to try for direct hits. The postcognitive modes were associated with one principal agent. "Clairvoyance" or "slow rate" subsittings never produced significant scoring (with one exception, and that in the "wrong" mode). The "relevant mode" for scoring is thus clearly defined by the experimental conditions; "clairvoyance" or "slow rate" conditions constitute "null" experimental conditions.

The computer DS and IDS searches covered 11352 digits, comprising all the PRN (8770 digits including 600 WN) plus most of the counters (2582 digits, Sittings 13, 14(5, 6), 17(4-10) excluded). Of the 8770 PRN, 1995 digits correspond to null experimental conditions.

In view of the DSs spotted bridging the Shackleton and Stewart data, it would have been interesting to have included a batch of Stewart data in the search, but this was not practicable with computer time increasing as the square of the quantity of data. A careful scan through Sittings 1-6 (telepathy runs) revealed no instances beyond the first few sheets however.

The work to be described was based on the Broad duplicates of the record sheets held by the SPR. The Scott-Haskell computer cards (Ref. 3, p. 47) punched from the Broad duplicates (and incorporating a number of corrections marked thereon in 1943) were used, but rearranged in strictly ascending order without regard to subsittings. Dr. Pratt possesses a typed copy of the Shackleton data which is believed to be closer than the Broad duplicates to the lost originals,† and Tables 2, 3, 5 have been verified by Dr. Platt in his records.

The method of the initial computer search for DSs (up to Sitting 24 only) consisted in stepping through the data at four-digit intervals, at each step searching all the preceding data, locating direct and reversed DSs of 12 or more digits with one discrepancy allowed.

Modifying the computer program to locate IDSs was a less simple matter. The procedure adopted was to consider 14 possible types of "interruption" in the compared sequences, namely:

1	2	3	4	5	6	7	8	9	10	11	12	13	14
*		*	**		**	*	**	***		***	*	****	
	*	*		**	*	**	**		***	*	***		****

† Reported by Dr. Soal to have been lost in 1945.

where ‡ represents a discrepancy (mis-match), and \* or \* represents an ED (non-match) in the first or second occurrence respectively of a DS. On encountering an "interruption" in the comparison process which could be considered as one (or more) of the 14 listed, it (or the one giving the "best" continuation) was accepted, and the procedure continued for up to three interruptions in each direction.

Table 1 lists the observed and expected frequencies for DSs of length 8 or more, in two categories: PRN only, and counters in one or both sequences (obtained by subtracting values for "PRN only" from values for full data). Expected frequencies are calculated from the formula:

$$E = \left(\frac{1}{5}\right)^n \frac{16}{25} (N-2n)(N-2n+1) \text{ (total for direct and reversed DSs)}$$

where  $n$  is the number of digits in the DS

$N$  is the number of digits in the search

**Table 1** DS frequencies (BS)

$n = \text{DS LENGTH}$	PRN ONLY				COUNTERS OR COUNTERS/PRN			
	DIRECT	REV	TOTAL	E	DIRECT	REV	TOTAL	E
8	69	69	138	125.6	38	49	87	85.0
9	28	13	41	25.1	8	3	11	17.0
10	6	5	11	5.02	2	1	3	3.40
11	3	2	5	1.003	-	1	1	0.85
12	1	1	2	0.201				(for $n \geq 11$ )
13	4	1	5	0.040				
14	4	2	6	$8.0 \times 10^{-3}$				
15	-	-	-	$1.6 \times 10^{-3}$				
16	3	-	3	$3.2 \times 10^{-4}$				
17	-	-	-	$6.4 \times 10^{-5}$				
18	2	-	2	$1.3 \times 10^{-5}$				
19	2	-	2	$2.6 \times 10^{-6}$				
$\geq 20$	6	4	10	$6.4 \times 10^{-7}$				

It is necessary to be able to estimate the probability of an IDS of closely with chance expectation, while the "PRN only" category shows a huge excess of long DSs. Incidentally, reversed DSs of 10 or more digits do not appear before Sitting 19.

It is necessary to be able to estimate the probability of an IDS of given form occurring by chance, in order that an objective criterion may be applied for selecting "significant" IDSs. No simple formula exists as in the case of ordinary DSs. Accordingly, an approximate formula was developed along the following lines, from consideration of the number of ways in which the interruptions in an IDS might arise.

Suppose the IDS to consist of:

M matching digits, taking not more than 11 from any one group  
r single EDs/discrepancies (interruption types 1, 2, 3)

s double EDs/discrepancies (interruption types 4, 5, 6, 7, 8)

with interruption types 9 to 14 treated as combinations of double and single interruptions. Then the expression:

$$m = M - \log_5 \left[ \frac{(M-6+r+s)!}{(M-6)+r+s!} (1.92)^r (3.2256)^s \right] \quad (I)$$

is taken as the "equivalent DS length", with the required probability estimated as though for a DS of length  $m$ .  $M-6$  matching digits are considered to provide for three "matches" at each end of the IDS.

At any stage in the comparison process, the next two digits in the two sequences could arise in 625 ways: of these 240 would result in a single interruption. Similarly, the next three digits in the two sequences could arise in 15625 ways: of these 4080 would result in a double interruption. From these ratios are derived the values 1.92 and 3.2256 for use in the combinatorial process represented by the expression in square brackets in (I). Taking the logarithm of the expression to base 5 gives the equivalent number of digits by which  $M$  should be reduced.

A criterion of  $m \geq 12$  was decided upon, comparatively low, it not being essential to exclude every "chance" case. However, on examining the ordinary DSs it was found that they rarely spanned two record sheets (and then only by a digit or two); moreover, about 60 per cent of the DSs were "in phase" (not more than one digit out of step with respect to the 25-digit columns). On the other hand, a number of IDSs with  $11 < m < 12$  were in phase and confined to one sheet, only one longer IDS ( $m=12.6$ ) spanning two sheets. Accordingly, in evaluating  $m$ , over-runs on to adjacent sheets were ignored (as being probably due to chance), and the criterion lowered to  $m \geq 11$  for IDSs in



phase at either end, probability considerations dictating the reduction of one unit.

Table 2 summarises the DS/IDS cases,† including all DSs of 11 or more digits and the IDSs selected according to the m-criterion. Cases 13, 26 are included being continuous with cases 12, 25 respectively, and hence clearly "non-chance". Cases 14, 24 are given as a matter of interest, but not included in the statistical analysis. Case 34, the only counters case, is probably a "chance" case (expectation 0.85 for DS length  $\geq 11$ ); however, the m-value of 13.2 led to a fuller check on the counters data to eliminate a possible substitution method of manipulation.

Cases 23, 24, 27-30 involve duplication between target lists allegedly prepared from Chambers's tables (earlier sequence) and Tippet's tables (later sequence). Case 33 appears to link the Soal and Wassermann PRN. Cases 2, 35, 39 bridge the Shackleton and Stewart data. In a number of cases duplication occurs within the same sitting.

Cases 14 and 31 are of special interest in view of the GA allegation concerning Sitting 16: the extra 4's in case 14 and the extra 5 in case 31 all correspond to hits.

Table 3 quotes the eight most important IDS cases in full, a study of which will clarify the meaning of the IDS formats in Table 2.

Table 4 extracts from Table 2 DSs of 13 or more digits and compares the scoring rates on first and subsequent appearances. The  $\chi^2$  tests show that the scoring rate does not decrease significantly in the subsequent appearances (assumed manipulation-free). This may not tell us much if, for example, manipulation was confined to regions sparse in hits. The scoring rates are comparable with those in the data as a whole. Expected rate of scoring E = 20 per cent.

$\chi^2$  tests (Yates' correction, 1 d.f.), single-tailed:

non-null:  $\chi^2 = 1.3487$  P=0.12, mean scoring rate=29.8 per cent.

null:  $\chi^2 = 0.0068$  P=0.47, mean scoring rate=18.5 per cent.

combined:  $\chi^2 = 0.2507$  P=0.31, mean scoring rate=26.2 per cent.

Table 5 amplifies the selected IDS cases from Table 2, setting out the EDs and discrepancies with hits and misses distinguished, plus other relevant information. Bracketed portions of the IDS formats are not considered "strong" enough to warrant including the EDs.

Table 6 classifies the EDs, and the hits thereon, into various categories with respect to digits 1-5. Binomial probabilities (valid

† A copy of the full computer print-out is held at the SPR.

**Table 2** Summary of DS/IDS cases

C:	clairvoyance experiment (normal rate)
T:	telepathy experiment (normal rate)
T(S):	telepathy experiment (slow rate)
*:	significant scoring (in the relevant precognitive, direct or postcognitive mode) for the subsitting concerned
DS Length:	number of matching digits including any "over-run" (1 or 2 digits as indicated). For clarity the DS length is placed opposite the second member of a DS pair.
IDS Format:	"base" figures indicate the <i>number</i> of matching digits between interruptions; indices, suffices, represent <i>actual</i> digits (1-5) forming interruptions in the upper, lower, sequence respectively. The format is placed opposite that member of an IDS pair most appropriate to Table 5.
m:	equivalent DS length (from formula (I) in text)
†:	included for reasons given in text
‡:	(k) signifies block of k unmatched digits in one sequence
X:	DS/IDS in phase (defined in text)

CASE	SITTING (sheet)	C/T	MODE	DS LENGTH		IDS FORMAT	m
1	1(3b) 1(4a)	C T	+1 +1*	13	X X	6 <sup>5</sup> 13	15.1
2	3(2a) 3(4a) GS 1(3a)	T T T	+1* +1* 0*	13 15		3 <sup>14</sup> 13 4 <sup>1</sup> (2 <sub>2</sub> 2 <sub>1</sub> )14(†2)	(11.7) (11.9)
3	1(4a/b) 5(4b/5a)	T T	+1* +1*	9(14)10 ‡			
4	5(4a/b) 5(6a)	T T	+1 +1*	24			
5	1(5a) 5(6b)	T T	+1* +1*	22	X		
6	4(4a/b) 6(1a/b)	T T	+1* +1*	16(7)8 ‡			

CASE	SITTING (sheet)	C/T	MODE	DS LENGTH		IDS FORMAT	m
7	1(2b)	T	+1*				
	6(6a)	T	+1*	25	X		
8	1(2a)	T	+1*				
	6(6b)	T	+1*	19-1	X		
9	7(3b)	T	+1*				
	7(7a)	T	+1	13			
10	6(1a) REV	T	+1*				
	12(3b)	T	+1		X	7 <sub>2</sub> 3 <sub>2</sub> 2,3( <sup>3</sup> 2 <sub>2</sub> )	11.2
11	21(4a)	T(S)	0				
	21(4b)	T(S)	0	18	X		
12	2(5a)	C	0		X	4 <sup>4</sup> 8 <sub>3</sub> <sup>25</sup> 4	11.8
	22(5a)	T(S)	0		X	10 <sup>4</sup> 3	11.3
	22(7b)	T(S)	0				
13	2(5b)	C	0				
	22(5b)	T(S)	0		X	4 <sub>4</sub> 8	10.4 †
14	16(2b/a) REV	T	+1*		X	3 <sup>3</sup> 3 <sup>4</sup> 3 <sup>4</sup> 6	(10.4) †
	22(6a/b)	T(S)	0				
15	23(3a)	T	+1*		X	11 <sup>1</sup> 4 <sup>2</sup> 4 <sup>1</sup> 3	16.5
	23(5a)	T	+1*	11	X		
16	23(3b)	T	+1*		X	12 <sup>1</sup> 4 <sup>2</sup> 3( <sub>23</sub> <sup>5</sup> 3)	14.6
	23(5b)	T	+1*	12	X		
17	24(1a) REV	T	+1				
	25(1a)	T	+1*		X	4 <sub>1</sub> 4 <sub>2</sub> 6 <sub>3</sub> 8	16.5
18	24(1b) REV	T	+1				
	25(1b)	T	+1*		X	10 <sub>5</sub> 6,5	17.1

CASE	SITTING(sheet)	C/T	MODE	DS LENGTH		IDS FORMAT	m
19	24(4a) REV 25(2a)	C T(s)	+1 0	14	X	4 <sub>1</sub> 5 <sup>152</sup> 12	13.7
20	24(4b) REV 25(2a/b)	C T(s)	+1 0	12-2	X X	10 <sup>1</sup> 1 <sup>35</sup> 9	15.5
21	24(2a) REV 25(3a)	C T	+1 +1*	11	X	3,3 <sub>2</sub> 11 <sup>24</sup> 6	16.4
22	24(2b) REV 25(3b)	C T	+1 +1*	13	X	13 <sub>4</sub> 3 <sub>5</sub> 1,3	13.0
23	20(1a) REV 25(4a)	T T(s)	+1* 0	14	X	13 <sub>21</sub> 9	17.6
24	20(3b) REV 25(4b)	T T(s)	+1* 0	5(5)7 †	X		
25	24(3a) REV 25(5a)	T T	+1 +1*		X	8 <sub>2</sub> 3 <sub>3</sub> 3 <sub>1</sub> 4	13.0
26	24(3b) REV 25(5b)	T T	+1 +1*		X	4 <sub>2</sub> 6 <sub>34</sub> 3 <sub>5</sub> 1,3	9.7 †
27	21(5b/a) REV 25(6a)	T T(s)	+1* 0	23-2	X	21 <sub>2</sub> 3	12.2
28	21(6b/a) REV 25(6b)	T(s) T(s)	0 0	27-2	X		
29	21(7a) REV 25(7a)	T T	+1* +1*	25	X		
30	21(8b) REV 25(7b)	T(s) T	0 +1*		X	10 <sup>55</sup> 4 <sub>1</sub> <sup>2</sup> 6(32)	15.5

CASE	SITTING (sheet)	C/T	MODE	DS LENGTH		IDS FORMAT	m
31	16(6a) REV 26(4a)	T C	+1* +1		X	(2 <sub>2</sub> )4 <sub>1</sub> 5 <sup>5</sup> 3 <sub>2</sub> 5	12.4
32	26(5a/b) 26(6a/b)	T C	+1* +1	11	X X	3 <sub>5</sub> 2 <sub>2</sub> <sup>4</sup> 11( <sup>2</sup> 43)	12.8
33	7(2a/b) WN 27(1a)	T T	+1* +1*	18			
34	WN 27(8b/a) REV 29(1a/b)	C T	+1 +1	11	X	4 <sub>1</sub> 11 (counters)	(13.2)
35	1(6b) 5(3b) 31(1a) 31(5a) GS 1(5a)	C T T T(s) T	+1 +1 $\pm 1_*$ 0 0*	14 13, 14 14, 16, 14	  X X X	   4 <sub>4</sub> 1 <sub>5</sub> 1, 10	11.3
36	2(2a) 4(2b) 32(4a) REV	T T T	0* +1* $\pm 1_*$	11 21, 11	X X	3 <sup>3</sup> 10 <sub>11</sub> 1 <sup>4</sup> 4	12.0
37	1(6a/b) 5(2b) 32(4a/b)	C T T	+1 +1 $\pm 1_*$	26-1 16	X		
38	3(1b) 33(4a)	T T	+1* $\pm 2_*$		X	9 <sup>4</sup> 5	11.9
39	4(5a/b) 6(3b) 32(2a/b) 36(1b) GS 2(1b)	C T T T T	+1 +1* $\pm 1_*$ $\pm 1_*$ 0*	19-1 23, 22-1	   X X	6 <sub>3</sub> 3 <sub>4</sub> 1 <sub>5</sub> 6 4 <sub>4</sub> 1 <sub>33</sub> 8 <sup>4</sup> 7	11.3 13.4

**Table 3** Eight IDS cases quoted in full

Notes. C, T, \* as defined in Table 2

Dotted digits represent hits in relevant mode

Sequences spaced out to display EDs

Matching digits underlined

For convenience, the earlier sequence of a "REV" pair is given in reverse order, but this should not be taken to imply anything about the "true" order of the sequences.

CASE	SITTING (sheet)	C/T	MODE	TARGET SEQUENCES (25-DIGIT COLUMNS)
15	23(3a)	T	+1*	35 <sup>3</sup> 4315445513514234321432
	23(5a)	T	+1*	<u>35343154455</u> 3514 <u>3432</u> <u>432215</u>
16	23(3b)	T	+1*	441343225514151142454 5122
	23(5b)	T	+1*	<u>441343225514</u> 5114 <u>454231221</u>
17	24(1a) REV	T	+1	5143 2532 543251 14232154343
	25(1a)	T	+1*	<u>5143125322543251314232154</u>
18	24(1b) REV	T	+1	3252142344 433425 355245131
	25(1b)	T	+1*	<u>3252142344543342513552411</u>
21	24(2a) REV	C	+1	234 441 1413215422424353155
	25(3a)	T	+1*	<u>2341441214132154224</u> 353155
22	24(2b) REV	C	+1	4353542233211 112 3 34214342
	25(3b)	T	+1*	<u>4353542233211411253134221</u>
25	24(3a) REV	T	+1	55541242 455 145 35255221143
	25(5a)	T	+1*	<u>5554124224553145135254324</u>
26	24(3b) REV	T	+1	541223 424415 111 3 553141413
	25(5b)	T	+1*	<u>4512232424415341115315532</u>

**Table 4** Comparison of scoring rates in DSs

Notes. In determining the "effective DS length", ends of DSs over-running on to adjacent sheets are omitted; also discounted are the void first, last, digits of columns in "+1", "-1", mode respectively.

Where the relevant modes are " $\pm$ ", the mean number of hits is taken.

CASE	FIRST APPEARANCE OF DS			SUBSEQUENT APPEARANCE(S) OF DS		
	EXPT'L CONDITION	EFFECTIVE DS LENGTH	SCORE IN RELEVANT MODE	EXPT'L CONDITION	EFFECTIVE DS LENGTH	SCORE IN RELEVANT MODE
1	null	13	2	non-null	13	6
2	non-null	15	5	"	13	0
				"	15	4
4	"	23	3	"	24	8
5	"	21	6	"	21	5
6	"	16	8	"	15	2
7	"	24	8	"	24	11
8	"	17	8	"	18	8
9	"	13	4	"	12	1
11	null	18	5	null	18	2
19	"	14	2	"	14	0
22	"	13	3	non-null	12	3
23	non-null	14	5	null	14	4
27	"	21	5	"	21	2
28	null	25	4	"	25	9
29	non-null	24	6	non-null	24	4
33	"	17	6	"	18	6
35	null	14	0	"	14	5
				"	13	3 (mean)
				null	15	2
36	non-null	21	10	non-null	20	4 (mean)
37	null	23	5	"	24	6
	"	11	5	"	15	3 (mean)
39	"	22	3	"	18	5
				"	25	10 (mean)
TOTALS:						
	non-null	226	74	non-null	338	94
	null	153	29	null	107	19
		379	103		445	113

**Table 5** Summary of EDs and discrepancies

Notes. Case and sub-case numbers relate to Table 2

Dotted digits represent hits in relevant mode

Underlined EDs are "unambiguous"

Ambiguous ED: ED not uniquely defined, e.g. belonging to target-pair

Apparent omission: missing digit in *one* member of a multiple DS

CASE	EXPT'L CONDITION	EDs	UNAMBIGUOUS EDs		APPARENT DISCREPANCIES & OMISSIONS
			HITS/TOTAL	MISSSES: TARGET-GUESS	
1.1	null	<u>5</u>	0/1	5-1	
10.2	non-null	<u>2</u> <u>2</u> <u>1</u>	1/3	2-4, 1-3	
12.1	null	<u>5</u>	1/1		2:3
.2	"				1:4
.3	"				4 (omission)
13.2	null	<u>4</u>			
15.1	non-null	<u>1</u> <u>2</u> <u>1</u>	3/3		
16.1	non-null	<u>1</u> <u>2</u>	2/2		
17.2	non-null	<u>1</u> <u>2</u> <u>3</u>	2/2		
18.2	non-null	<u>5</u> <u>1</u>	2/2		
19.1	null	<u>152</u>	0/1	1-5	
.2	"	<u>1</u>	0/1	1-3	
20.1	null	<u>1</u> <u>35</u>	0/3	1-4, 3-2, 5-1	



MAY 1978]

*The Soal-Goldney Experiments*

CASE	EXPT'L CONDITION	EDs	UNAMBIGUOUS EDs		APPARENT DISCREPANCIES + OMISSIONS
			HITS/TOTAL	MISSSES: TARGET-GUESS	
21.1	null	$\dot{2}\dot{4}$			
.2	non-null	$\dot{1} \quad \dot{2}$	2/2		
22.2	non-null	$\dot{4} \quad \dot{5} \quad \dot{1}$	3/3		
23.2	null	$\dot{2}\dot{1}$	0/1	1-3	
25.2	non-null	$\dot{2} \quad \dot{3} \quad \dot{1}$	1/2	1-3	
26.2	non-null	$\dot{2} \quad \dot{3}\dot{4} \quad \dot{5} \quad \dot{1}$	3/5	2-1, 3-5	
27.2	null	$\dot{2}$	1/1		
30.1	null	$\underline{55}$	0/2	5-3, 5-4	
.2	non-null				$\dot{1}:2$
31.1	non-null	$\dot{5}$	1/1		
.2	null	$\dot{1} \quad \dot{2}$	0/2	1-3, 2-4	
32.1	non-null				$\dot{4}:2$
.2	null	$\underline{5}$	0/1	5-2	
35.5	non-null	$\dot{4} \quad \dot{5} \quad \dot{1}$	1/1		
36.2	non-null	11			3, 4 (omissions)
38.1	non-null	$\underline{\dot{4}\dot{4}}$	1/2	4-2	
39.4	non-null	$\dot{3} \quad \dot{4} \quad \dot{5}$			
.5	"	$\underline{\dot{4}\dot{1}} \quad \underline{\dot{3}\dot{3}}$	2/4	1-5, 3-1	4 (omission)

**Table 6** Frequencies of digits 1-5 among the EDs

F: frequency of occurrence of digit 1, 2, 3, 4, 5

H: corresponding number of hits

Singles: ED groups excluded

	1		2		3		4		5		TOTAL	
	H	F	H	F	H	F	H	F	H	F	H	F
BS 24/25	5	6	3	4	2	3	2	2	3	3	15	18
Other BS	3	6	3	4	1	1	2	3	2	2	11	16
GS 1,2	1	2	0	0	1	2	2	2	1	1	5	7
Non-null total	9	14	6	8	4	6	6	7	6	6	31	41
Null	0	5	2	5	0	1	2	2	1	7	5	20
TOTAL	9	19	8	13	4	7	8	9	7	13	36	61
UNAMBIGUOUS EDs ALONE												
BS 24/25	5	6	1	2	2	3	2	2	3	3	13	16 <sup>(1)</sup>
Other BS	3	4	3	4	0	0	1	2	1	1	8	11
GS 1,2	1	2	0	0	1	2	1	1	0	0	3	5
Non-null total	9	12	4	6	3	5	4	5	4	4	24	32 <sup>(2)</sup>
Null	0	5	1	2	0	1	0	0	1	6	2	14
TOTAL	9	17	5	8	3	6	4	5	5	10	26	46 <sup>(3)</sup>
											22	32 <sup>(4)</sup>

Binomial probability computations:

- (1) 13 or more hits in 16 trials:  $P=2.48 \times 10^{-7}$
- (2) 24 or more hits in 32 trials:  $P=3.21 \times 10^{-11}$
- (3) 26 or more hits in 46 trials:  $P=5.30 \times 10^{-8}$
- (4) 12 or more hits in 14 trials:  $P=2.48 \times 10^{-7}$
- (5) 20 or more hits in 24 trials:  $P=4.79 \times 10^{-11}$
- (6) 22 or more hits in 32 trials:  $P=3.25 \times 10^{-9}$

only for unambiguous EDs) are given where appropriate—the most notable being that for total non-null unambiguous EDs:  $P=3.21 \times 10^{-11}$ . The striking difference between the non-null and null totals should be noted, the latter purely at chance level.

A target-guess matrix compiled for EDs would be too sparse to allow any conclusions to be drawn. It is perhaps of interest that digit 1 tends to predominate in both hits and misses; also, the combination 1-3 is most frequent among the misses.

The Soal-Goldney Report (Ref. 7, p. 132-4) notes a highly significant difference in success rates on the five animal symbols, greatest on E and Z, least on L (for  $\chi^2=36.6$ , 4 d.f.:  $P<10^{-6}$ ). Because of the ease with which L's could be converted into E's and Z's, it seemed of interest to check the frequencies of E, G, L, P, Z among the EDs: no suspicious trends are present however.

Table 7 examines the IDSs (non-null conditions) for residual significance when the ED trials are discounted. It will be seen that the scoring rate drops to near-chance levels—from 30.4 per cent to 23.5 per cent on the grand total (E=20 per cent)—with the binomial P-value soaring from  $9 \times 10^{-6}$  to 0.09.

It is instructive to study the Sitting 25 case in more detail. Sheets 1, 3, 5, 7 form a "telepathy" subsitting, sheets 2, 4, 6, 8 a "clairvoyance" subsitting (i.e. null). For the telepathy subsitting 54 hits were recorded,  $E=38.4$ , deviation=15.6: about 2.8 standard deviations. Sheets 1, 3, 5 contain 18 EDs (none in sheet 7) of which 15 correspond to hits; on removing the 18 suspicious trials from the total the deviation is reduced to an insignificant 4.2: about 0.8 standard deviations. The four null sheets contain only 4 EDs.

The presence of EDs in null conditions possibly served to adjust digit-frequencies; such EDs tend to occur in groups. Further, Soal would not necessarily have known which PRN sheets were to be used under null conditions, especially if (as appears) the sheets were not numbered in advance (Ref. 7, p. 39).

In Ref. 3 (p. 65) Scott and Haskell speculate that Soal might have worked the manipulation "systematically one sheet behind"—but that there was no sign of "a fall to chance scores on the last sheet preceding check-up in each PRN sequence", which thus argued against the hypothesis. However, examination of the 19 *significant subsittings following a major change in experimental procedure* (see Part 3) does reveal a *sudden fall* in scoring rate (in the relevant modes) on the final sheets:

		Hits	Misses	Total
Sittings 19-40 (significant subsittings)	Final sheet	294	850	1144
	Other sheets	1009	2209	3218
Total		1303	3059	4362

$\chi^2=12.62$  (Yates' correction, 1 d.f.):  $P=1.9 \times 10^{-4}$  single-tailed.

**Table 7** Residual significance in IDSs (non-null)

Notes. Case and sub-case numbers relate to Table 2

In "+1" mode the void first digits of columns are discounted

		a	b	c	d	a-c	b-d
CASE	MODE	IDS LENGTH	HITS IN IDS	EDs	HITS ON EDs	REDUCED IDS LENGTH	REDUCED HITS
BS 24/25							
17.2	+1 *	24	6	3	3	21	3
18.2	+1 *	22	5	2	2	20	3
21.2	+1 *	24	7	2	2	22	5
22.2	+1 *	22	11	3	3	19	8
25.2	+1 *	20	6	3	2	17	4
26.2	+1 *	22	5	5	3	17	2
SUB-TOTAL		134	40	18	15	116	25
Other BS, 65							
10.2	+1	18	4	3	1	15	3
15.1	+1 *	24	8	3	3	21	5
16.1	+1 *	20	8	2	2	18	6
31.1	+1 *	18	8	1	1	17	7
35.5	0 *	19	6	3	3	16	3
36.2	+1 *	19	8	2	0	17	8
38.1	+1 *	15	2	2	1	13	1
39.4	+1 *	18	6	3	3	15	3
39.5	0 *	24	4	4	2	20	2
GRAND TOTAL		309	94	41	31	268	63
		$P = 9.02 \times 10^{-6}$				$P = 0.0889$	

MAY 1978]

*The Soal-Goldney Experiments*

it is interesting to note that Sitting 25 sheet 7 is in line with this supposition, with a chance level 11 hits, and no EDs in the duplication.

In concluding, it may be remarked that although the criterion used in the selection of IDSs may be imprecise, this is not crucial; a much cruder criterion (sum of matching digits less interruptions) leads to only minor changes on the borderline. Likewise, whether or not one includes various qualifications which have been mentioned has little effect on the estimated significance. This demonstrates the *stability* of the statistical analysis, insensitive to borderline fluctuations.

When one considers the systematic duplication between Sittings 24/25 and the phenomenal scoring rate on the EDs, it is scarcely necessary to resort to statistical analysis to establish that a serious anomaly exists. The interpretation of that anomaly is perhaps another matter.

### PART 3 DISCUSSION: INTERPRETATION OF THE EVIDENCE

How is the evidence presented above to be interpreted?

The long duplicated sequences in the target lists—while scarcely enhancing Soal's reputation for meticulousness—are not in themselves suspicious, and could easily have arisen from repeated use of a smallish pool of quasi-random digits.

The duplications linking PRN *allegedly* taken from two distinct sources (Chambers's Logarithm Tables and Tippett's random number tables) show that, at the very least, no reliance can be placed on the sources cited by Soal; at worst, they could be construed as part of a "cover-up" operation.

When one comes to the *interrupted* duplicated sequences (IDSs) with their suspicious extra digits (EDs), then, on the face of it, the evidence seems consistent with the hypothesis advanced by Scott and Haskell in Ref. 3, p. 52: namely, that the target lists were pre-stacked with 1's for subsequent alteration to other digits in order to secure spurious hits. Except that the new evidence presents the manipulation effect directly, pinpointing specific digits. It is curious, however, that only *inserted* digits appear to be affected. Why should 1's forming part of the true sequence be immune? Alteration of a digit in the true sequence would show up as a discrepancy (mis-match) in the IDS—but they are almost entirely absent (as may be seen from Table 5), the exceptions readily explainable as copying errors.

Suppose however that, to save effort, Soal occasionally prepared target lists by copying from earlier record sheets, tending to avoid digits which had corresponded to hits—as a compulsive-type personality might well do. The effect would be to produce duplicated sequences with apparent insertions corresponding to hits. Plausible enough, did not most of the cases presented in Part 2—in particular the impressive Sitting 24/25 complex—exhibit the suspicious extra digits in the *later* sitting of the pair; while in the Sitting 23 case a single sitting is involved.

Returning to the peculiarity mentioned above—that digits forming part of the true sequence appear to be immune—various hypotheses might be advanced to account for this. Thus, Soal might have left gaps when preparing the target lists (in ink), then carefully pencilled in 1's for subsequent manipulation. Unfortunately, of course, the original records which might have been examined for signs of erasure are not available. Alternatively, the inserted digits (not restricted to 1's) might have served to mark positions for pre-arranged or manipulated guesses (according to whether or not Soal was in collusion with Shackleton).

Manipulation of the pencilled guesses seems a plausible hypothesis. Especially might a switch to guess-manipulation be suspected with Soal's change of role from EA (experimenter controlling agent) to EP (experimenter controlling percipient), i.e. Soal would have been in charge of the guess-sheets. The "Chronicle of Experiments" (supplement to Ref. 7) records that another innovation was introduced on the same occasion—Sitting 19—with Shackleton indicating his guesses and Soal recording them. For most of the remaining sittings Soal recorded the guesses—usually entirely unobserved or observed in an intermittent fashion. In Sitting 24 alone does the observation of Soal as EP appear to have been continuous, and that sitting yielded only chance results.

In Ref. 4 (p. 142) Soal admits "the slight drawback that errors might occasionally arise, through inaccurate recording by (EP) of Shackleton's choice", but points out that: "In most of the forty sittings, Shackleton recorded his own guesses", and that at those sittings highly significant results were obtained. But what if the admittedly possible "inaccurate recording" should have become *mis*-recording? Soal was EP at 19 significant subsittings, recording Shackleton's guesses at 16 of them (as ascertained from the "Chronicle of Experiments")—amounting to a substantial 40 per cent of the total significant subsittings.

Manipulation of target digits is virtually ruled out as an explanation of the high scoring in certain non-random runs:

- (i) Three special non-random tests, each consisting of two blocks of repeated digits (12 of one digit followed by 13 of another).
- (ii) PRN compiled by C. U. Blascheck for Sitting 28, and grossly non-random: Soal has noted on the back of one of the Broad duplicates that Blascheck must have been under the misapprehension that repeats should be omitted when encountered—it seems unlikely that these target lists could have been manipulated without tell-tale pairs arising.

I shall not dwell on *how* the supposed manipulation might have been effected, or whether it could have been done single-handed—save to emphasise that a number of manipulation methods could have been devised to suit particular experimental conditions, and might well have included working “one sheet behind”. A further observation: the non-null subsittings of Sittings 1–17, with the principal agent R.E., constitute an unbroken run of significant scoring; Soal is informed of the GA allegation after Sitting 17; Sitting 18 yields chance results; at Sitting 19 Soal changes his role to EP.

Protestations to the effect that Soal, a respected scientist, would not have cheated in his own experiments—and that anyway the rigorous experimental conditions in the Shackleton series precluded fraud—seem to me to carry little weight in the face of the evidence. We can rarely fathom how conjurors achieve *their* feats, and perhaps Soal was as clever. It is futile to argue that the prison cell is escape-proof when the inmate has clearly gone.

On the other hand, certain *irrational features* in the duplication raise doubt as to whether the manipulation took the form of fully conscious cheating. For example, some duplications occur at dangerously close intervals, while the systematic reversal between Sittings 24 and 25 seems quite unnecessary with an extensive random number table such as Tippet's available—duplication without which the damaging evidence could never have come to light.

I shall therefore offer two hypotheses, of contrasting nature, based on the assumption that manipulation did occur: frankly speculative yet, I suggest, in accord with the known facts, psychological and otherwise. It is not necessarily a question of *either* Soal cheated *or* he did not. Hypothesis (A) may seem rather bizarre, but bearing in mind Soal's bizarre personality it need not be dismissed out of hand on that account.

#### *Hypothesis (A): Dissociated Manipulation*

It is clear from the literature, and from the comments of those who

knew Soal personally, that his was indeed a strange personality: obsessive, absorbed, secretive, and subject to bouts of dissociation. Soal himself speaks of having been "afflicted with the mental disorder of automatism". (Ref. 8, p. 176), and of how (p. 174) "in certain individuals" the mind may possess strong undercurrents unusually organised and active, and that "these mental streams cut off from the main river of conscious life [may] find an outlet by means of automatic writing."

Soal was an accomplished automatist, as the remarkable "Oscar Wilde" and "Margaret Veley" scripts testify (Ref. 9). On p. 332, writing under the pseudonym *Mr. V.*, Soal describes the process thus:

"By concentrating my mind on some trivial calculation I have learnt how to create a mental state of intense distraction, and it is during the moments when my mind is thus occupied that the writing is produced. The moment my mind reverts to what my hand is supposed to be doing the writing stops as suddenly as it commenced."

Even more relevant to the purpose in hand is the purporting communicator Mr. "X". "X" persistently refused to disclose his identity, but on one occasion referred to having "seen the Snowdon mountains through the eyes of this automatist" i.e. Soal (Ref. 9, p. 368). Questioned as to whether he was the subconscious mind of the automatist, "X" indignantly denied this—but Soal clearly *did* so regard him, commenting (p. 369):

"... there is no need to think of 'X' as anything else than my own subconscious activity..."

The "communications" of "X" consisted mainly of verse, sustained subliminal creations of considerable beauty of form and imagery—unconscious productions of Soal's hand. Is it too fanciful to speculate that "X" might have had a "hand" also in the production of the experimental record sheets? Although Soal claimed to have discontinued the practice of automatic writing by the time of the Shackleton experiments, the tendency persisted (as shown by an incident described by Mrs. Goldney in the June 1975 *SPR Journal*, p. 97). Denial of expression could have led to "X" manifesting in a more repressed manner.

Imagine Soal, alone in his room, entering the target sequences on record sheets in readiness for the next sitting: pen in hand poised above the record sheet; other hand pointing to a place in the random



number pool. At intervals Soal glances towards the pool to read off the next few digits: his attention thus distracted from the writing hand it would be easy enough for "X" to slip in a digit or two of his own.

One possible innocent explanation of the "extra digit" anomaly which has been offered considered that Soal might unwittingly have been acting as his own precognitive subject while preparing the target lists, unconsciously departing from the true random sequence to produce above-chance agreement with Shackleton's future guesses. This seems far-fetched in view of the phenomenal scoring on the extra digits—unless the entity "X" is to be credited with superior paranormal powers.

More plausibly, and taking the dissociation hypothesis to the limit, is it not conceivable that a secondary personality of Soal—in the manner of a Sally Beauchamp—carried out the falsification of the experimental results unknown to the main personality? This could account for the irrational features in the duplication previously mentioned.

It is generally accepted that even ostensibly genuine mediums may, when opportunity allows, resort to deception to supplement their powers, *especially in a dissociated state*. Experimenters of a mediumistic type, as Soal seems to have been, may be subject to similar pressures.

#### *Hypothesis (B): Data Massage*

Psychological objections have been raised that Soal, a respected scientist, would hardly have perpetrated a hoax, persevering for years in a charade of research, then, having won recognition, unnecessarily have risked exposure in further series of experiments, finally labouring for another four years on an entirely null series. Respected scientists *have* been known to perpetrate hoaxes however—the Piltdown Skull scandal to name but one.

In September 1976 *New Scientist* began an investigation into "intentional bias" in science (Ref. 10). The author comments that: "It seems unlikely that many researchers set about their experiments with deliberate deception in mind." When things go wrong however, with results persistently negative and pressures mounting "... the temptation to 'improve' the results slightly must be very strong."

In October 1976 the Sir Cyril Burt scandal broke, with allegations that he had fabricated research data to fit his preconceptions. Much has since been heard of "data massage" in scientific reports. We have been "regaled" by the spectacle of a biochemist publicly confessing

that he had falsified his experimental results to support a hypothesis he considered particularly important (cf. statement in *Nature*, 24 Feb. 1977, p. 764). (Ironically, that hypothesis appears to have been subsequently confirmed by other researchers.) In the vulnerable field of Parapsychology, of course, we have the distressing case of Dr. Levy at Rhine's laboratory: in 1974 Dr. Levy admitted to having improperly bolstered a research project he especially wanted to keep going successfully (Ref. 11).

A particularly insidious form of cheating arises where malpractice is resorted to, not for personal gain, but for the furtherance of a cherished cause or theory, perhaps believed on intuitive grounds. The perpetrator indeed may fail to recognise the cheating as such: a betrayal of scientific method, no less, however motivated. In these circumstances, the psychological objections raised as to motivation become irrelevant.

During the years 1934-9 Soal tested 160 subjects, amassing 128,350 trials, with null results (Ref. 12). Subsequently, at the insistence of Whately Carington, Soal re-examined his records for displacement effects, and this led to the unexpected discovery of two high-scoring subjects: Basil Shackleton and Gloria Stewart. Conceivably this work was entirely genuine. Presumably, too, Soal's earlier negative experimental work, such as the 1927-9 series so painstakingly recorded and analysed in Ref. 8, must be considered genuine.

Having embarked upon the Shackleton series, one may imagine the scoring rate begins to fade (as ESP scores are wont to do after the initial flush of success). Soal, seeing the chance slipping away of gaining scientific recognition for Parapsychology, a cause in which he passionately believes, succumbs to the temptation of "rectifying" a "temporary" deficiency. In later years, perhaps, his work now established, he reverts once more to trying for genuine results—after all, there is nothing to lose, a null series could even enhance the apparent genuineness of the earlier research. Thus might be explained the unsuccessful series carried out at Birkbeck College during the years 1954-8.

In the final analysis personal experience is the crucial factor. Experience alone, however impeccable, does not convert the sceptic—and understandably so. Yet once the sceptic's eyes are opened by experience no amount of revelation of faked experiments can undermine the conviction that there remains *something* to be investigated. One hopes that open-minded scientists will continue to investigate that something, seeking to establish its true nature be it fact or fantasy.

## CONCLUSION

It is submitted that the evidence presented in this paper establishes, in the absence of a convincing "innocent" explanation, the existence of manipulation in certain sections of the target lists of the Shackleton data. Whether or not the manipulation resulted from fully conscious cheating and whatever the psychological motivation, and even though the ESP effect may not be entirely due to manipulation (one may surmise that it is but this has not been demonstrated)—the sad and inescapable conclusion remains that all the experimental series in card-guessing carried out by Dr. Soal must, as the evidence stands, be discredited.

## CODA

I record here, for what it is worth, the fact that the initial motivation for the above piece of research sprang from a dream: a dream of a most intense quality in which Dr. Medhurst appeared in the role of tutor explaining a mathematical/graphical problem which he wished me to work upon. The dream occurred on 31 March 1971; five days later the *SPR Journal* arrived containing, to my astonishment, a posthumously-published paper by Dr. Medhurst, the subject matter of which seemed curiously linked with my dream. While shunning a survivalist interpretation, it was difficult to resist the feeling that an element of ESP might nevertheless be involved, impelling me to follow up certain ideas suggested by the dream—with the outcome reported in this paper.

Incidentally, apart from attending (in 1969) two SPR lectures led by Dr. Medhurst, I met him only once, briefly, in 1963.

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## STATEMENT BY MRS. K. M. GOLDNEY

I have been in close touch with Miss Betty Markwick throughout her investigation of the Soal-Shackleton experimental data and would like to pay tribute to the remarkable findings she made in the 262 record sheets in the Shackleton files kept at the SPR. These many pages of figures had been minutely examined by Hansel before he wrote his book *ESP: A Scientific Evaluation* and also by Scott and Haskell before their paper in *Proc.* Vol. 56, Part 209, October 1974; and though both these publications were critical of and adverse to Soal, none of the three writers had noticed the particular series of repeated sequences in the target lists which Betty Markwick discovered. Hers was a truly remarkable observation.

Immediately following the Scott-Haskell paper, in October 1974, eight well-known investigators contributed articles in Soal's favour: myself, Professor Mundle, Professor Thouless, Dr. Beloff, Professor J. G. Pratt, Miss M. R. Barrington, Professor Ian Stevenson and Professor J. R. Smythies. Of these contributors defending Soal and his work, it was I who had had the closest knowledge of Soal in my long contact with him in experimental work since the 1930's and as co-author with him of the "Shackleton Report"—the paper entitled *Experiments in Precognitive Telepathy*, *Proc.* Vol. 47, Part 167, December 1943. If Miss Markwick's findings are valid (and I have no reason to believe otherwise), I and others who replied to the Scott-Haskell paper were wrong, but justifiably so, in my opinion, in the light of the evidence then available. On page 81 of my reply I wrote: "Of course an adverse verdict, if established, will destroy individual hopes, even beliefs. So be it, if necessary. 'The world is wide' and the object of all our studies is to find and establish *the truth*." This sentence summarised what I felt when I wrote it and what I feel now.

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**Postscript**

My offer remains open to bona-fide investigators to examine at my home my files on the experiments with Shackleton and the huge number of letters I received from Dr. Soal during this work.

### STATEMENT BY PROFESSOR J. G. PRATT

In my judgment Miss Markwick's report is an exemplary scientific account of a remarkable achievement in problem solving through data analysis. She was, of course, taking up the task at the stage where it was left by her predecessors, so she was in one sense in their debt. But whereas there were several persons who came forward to defend Dr. Soal against the charge of experimenter fraud put forward on the evidence previously offered, I shall be greatly surprised if there are any who will come forth to defend his work with the same vigour after learning of Miss Markwick's findings. My part in this latest advance toward unravelling this scientific mystery has been a minor one, but since Miss Markwick has been kind enough to mention my role I think the readers of the *Journal* may be interested in a few further clarifying comments from me.

(1) Over the years I made a considerable professional investment in efforts to extend the findings from the experimental records of the work with Basil Shackleton and Gloria Stewart. The results of these further analyses were reported in several articles by me alone or jointly with Soal. In connection with this work, Soal provided me with complete copies of his experimental data from these two subjects. There are indications that my typewritten set of the Shackleton data was largely derived from the original experimental records instead of from the handwritten copies made at the conclusion of each sitting and posted to Professor Broad.

(2) I checked Miss Markwick's detailed findings against my copy of the data and confirmed the results she obtained through her computer processing of the Broad copies now held at the SPR. I raised a few questions about what appeared to be minor discrepancies, but these were not such as to change in any way the import of Miss Markwick's findings, and in most instances the errors were mine. Thus there can be no doubting the fact that the effects she has pointed out really exist in the records.

(3) At the same time, her analyses leave unanswered many questions (as the report indicates) regarding the final interpretation and evaluation of the research. At the moment I agree with Miss Markwick that there may be no way by which the major part of the Shackleton and Stewart records that are not directly involved in her findings can be retained as valid evidence of ESP. Thus I regretfully agree that we must set aside, at least for the time being, all of the Soal experimental findings as lacking scientific validity. This is a severe

verdict, and some other workers may wish to challenge it on the basis that further analyses of the data could conceivably show that at least some of the results are still sound as evidence of psi. I will be glad to make copies of my records available at cost to qualified scientists who may wish to make further analyses of the data.

(4) Indeed, some investigators may think it is possible to defend on logical grounds and without further analyses some of the published findings from the Soal records as providing valid evidence of ESP in spite of the results that Miss Markwick has presented. Much of my own work with these records, particularly those from the Stewart series, brought to light position effects and displacement effects that I interpreted at the time as giving strong supporting evidence for paranormal processes at work in the tests. Similarly, Soal and I jointly reported upon some obscure but highly significant relations between call and target sequences that we interpreted as strong additional evidence for ESP. For the present I must put all of this work aside, marked to go to the dump heap. At the same time I can hope, just barely, that someone may find a way in which it can be salvaged. Does this case not provide an acid test for the claim that has been argued strongly in some of the recent literature of our field? This is the claim that such secondary effects as those mentioned above provide a kind of bedrock evidence for psi that is untouched even by accusations of fraud in the research.

(5) Finally, I wish to emphasize that what Miss Markwick's work clearly shows is that manipulations in the target sequences occurred—manipulations that are not only inconsistent with what the responsible experimenter stated in the published report but are also directly related to the positive scoring that occurred. She does not provide an unambiguous interpretation of her findings that would, for example, justify our concluding that Soal consciously cheated in his research. We are in the position, therefore, of having to accept the fact that there are anomalies in the data that cast serious doubts upon Soal's work as evidence for ESP, but we are not yet at the point that we can specify how and why the suspicion-arousing effects occurred. Those persons who have already formed the opinion that Soal cheated in his own experiments will certainly not feel any need to re-examine that belief in the light of the new evidence. On the contrary, they are likely to feel that their suspicions have been confirmed. But those of us who have difficulty reconciling the idea of conscious fraud with the picture of an investigator who was totally absorbed in his research will not be compelled by Miss Markwick's findings to overcome that reluctance and agree that Soal was a hoaxer. I do not mind revealing

that I am the person who suggested that Soal might have become his own subject on some occasions when preparing the lists of random numbers on the record sheets before the sittings were held. This explanation would require that he used precognition when inserting digits into the columns of numbers he was copying down, unconsciously choosing numbers that would score hits on the calls the subject would make later. For me, this "experimenter psi" explanation makes more sense, psychologically, than saying that Soal consciously falsified for his own records, but I do not argue that it should be accepted by others as the likely interpretation. This is the reason why I said earlier in correspondence with Miss Markwick that I did not wish to defend this explanation of how the high scoring on the inserted digits came about. To have done so would have made it seem that I was defending Soal's innocence as a necessary step toward defending the bulk of his work. That is not my position. Rather, I agree with Miss Markwick that, since some of the data are seriously deficient, we are compelled to consider all of the records to be invalid as evidence of ESP unless or until they can be shown to be otherwise. But as long as there are viable alternative explanations, both innocent and guilty, we cannot sit in judgement of Soal regarding his behaviour, motives, and character.

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